Reviewer’s report

Title: Exploring Social Cognition in Patients with Apathy Following Acquired Brain Damage.

Version: 4 Date: 28 August 2013

Reviewer: Lyn Turkstra

Reviewer’s report:

The authors have addressed several of the limitations of the first version of the manuscript, including providing further detail about measures, providing rationales for their decisions about several aspects of the methods, and adding a data-analysis overview section. The authors did not respond to several comments in the category of “major essential revisions”, or responded incompletely, so they are restated here.

1. The authors’ last point on page 4, about “subconscious emotional signals that pre-bias and influence goal-directed social behavior” needs expansion – i.e., how would this theory influence the likelihood that apathy is specifically related to social cognition or moral reasoning, other than the same argument that similar structures are involved?

2. The authors added a sentence to state that they used three tests of EFs because some participants had deficits that precluded testing with one or another of the three. This rationale implies that the tests are psychometrically interchangeable, which is not the case. The authors should describe the process of comparing scores from different tests.

3. The authors’ statement that, “We took ‘A’ responses to indicate that participants judged protagonists behaviour as normative” is not sufficient explanation for the scoring system. How many “healthy controls” provided “normative data”? What were the characteristics of the healthy controls and how were data collected? Why the 95% cutoff for agreement? Presumably the “normative” data (in quotes because it is not clear that standard procedures for norming a measure were followed) were not from the healthy comparison participants in the study, as that would mean that half of the items should not have been included because typical adults did not agree on answers.

4. The contribution of inadequate power to results should be addressed explicitly in the results, as non-significant findings often were in the expected direction (e.g., the large difference between healthy comparison/non-apathectic participants and those with apathy on the Ekman test, which does not make sense as stated, given that median values for healthy controls and non-apathectic patient groups were identical).

5. The authors do not state methods for addressing alpha slippage associated with the multiple comparisons shown in Table 6.
6. Memory and language test scores should be reported.

Minor essential revisions

1. Some statistical analysis results are still reported in a way that misrepresents the analysis – e.g., “Chi-square tests revealed that there was a significant the presence or absence of apathy in patients had an effect on whether or not patients correctly identified normative behaviour.” The statement “had an effect on” implies regression, although the sentence appears to be incompletely edited to this might be an oversight.

2. The authors are again reminded to use person-first language (e.g., patient with brain injury rather than brain-damaged patient).

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.